Atmos. Chem. Phys. Discuss., 11, C15286–C15289, 2012
www.atmos-chem-phys-discuss.net/11/C15286/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “CO$_2(\nu_2) - Oquenchingratecoefficientderivedfromcoincidental SABER/TIMEDandFo$

Anonymous Referee #2

Received and published: 14 February 2012

General comments

The paper address a long-standing problem in the middle/upper atmospheres of the terrestrial planets: the activation/de-activation of CO2(v2) in collisions with atomic oxygen, which is very important for the infrared cooling budget of these regions as well for the temperature inversion from CO2 15 μm radiancies.

In this paper, the authors derive this rate based on the SABER CO2 15 μm radiances, on the Fort Collins lidar temperatures and on the atomic oxygen abundances indirectly derived from other SABER channels. While they account for the mapping of SABER pressure and temperature noise errors below 80 km, and the lidar temperature instrumental noise, they do not propagate the systematic errors of these quantities and, most importantly, the systematic uncertainties of the atomic oxygen derived from SABER. This is a crucial point that affect to the accuracy of the derived rate, which is manifestly

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underestimated.

The authors did not properly acknowledge previous work done on this area. Thus, Remsberg et al., JGR (2008) validated SABER temperatures and already compared them with the Lidars measurements at Fort Collins used in this work (see Sec. 5.3. pp. 22-24). They found a good agreement between the Fort Collins lidars temperatures and SABER temperatures when considering the estimated 50% error in the atomic oxygen concentration and the collisional rate coefficient K(CO2-O) of 6e-12 cm3s-1 (see also Garcia-Comas et al., JGR, 2008). Hence, it is not surprising that the value the authors propose, 6.1e-12, is nearly identical to the value used in Remberg et al. of 6.0e-12. The only difference is that the error bar proposed now is smaller, 30% instead of 50%, but this is explained because the authors did not include the systematic error in the atomic oxygen indirectly derived from the SABER OH and O3 channels. Hence, what the authors are showing was essentially done before by comparing the SABER and Lidar temperatures. I really cannot see any progress in this study and hence I do not recommend its publication in ACP. First, it does not report any new result; and secondly, it does not include any systematic error in the atomic oxygen abundance when estimating the errors of the collisional rate.

In addition to the major points above, I would not recommend its publication for these other reasons:

The authors should have used the individual O profile derived in each of the coincidences (individual radiance calculations) when deriving each of the single collisional rate values. In principle, if the problem is linear, one would not expect significant changes in the mean value, however, there will be in the estimated standard deviation.

The proposed mechanism for the altitude dependence of the rate coefficient is very attractive but very speculative and it does not deserve 40 lines of text. The reader would like to see some estimations (even "back of the envelope" estimations) of the concentration of hot O and how compares K_hot*[O_hot] with K*[O]. I suggest to remove the whole paragraph if no estimations can be provided or to reduce it if they are given. On the other hand, it surprises that the authors did not invoke a probably more likely reason: the temperature dependency of K_CO-O. Its altitude dependency resembles very much that of the temperature profile.

In the review of K(CO2-O) measurements (Table 1) a key atmospheric derivation is missing, that done using ATMOS measurements (Lopez-Puertas et al., 1992). This is one of the most comprehensive derivation since there were measured, simultaneously, all the key magnitudes (pressure, kinetic temperature, CO2 vmr, and CO2(010) vibrational temperature) except the atomic oxygen. Since the review also cover theoretical studies, it might also be worthwhile to mention the study by De Lara et al., J. Chem. Phys., 2006.

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